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Minnesota. He has been granted a leave of absence for a year in order to take charge of the Research Division of the Gas Offensive at the American University in Washington. Dr. Harry S. Fry, associate professor, has been appointed acting head of the department of chemistry in the University of Cincinnati.

JOHN F. GUBERLET, A.M. ('11, Illinois), Ph.D., '14 (zoology), who since 1915 has been professor of biology at Carroll College, Waukesha, Wisconsin, has recently accepted the position of assistant parasitologist at the Oklahoma Agricultural and Mechanical College and Experiment Station, at Stillwater, Oklahoma. He will take up his work in Oklahoma on July first.

HERBERT RUCKES, in charge of the department of biology at Grove City College, has resigned to accept a position in the department of biology at the Agricultural and Mechanical College of Texas. For the past year Mr. Ruckes has been carrying on a botanical survey of Mercer county, Pa.

PROFESSOR H. V. TARTAR, who for the first five years has been station chemist and associate professor of agricultural chemistry at the Oregon Agricultural College, has accepted a position in the department of chemistry of the University of Washington at Seattle.

DISCUSSION AND CORRESPONDENCE

SOLUTION TENSION AND INDUCTIVITY

TO THE EDITOR OF SCIENCE: In SCIENCE of May 3, Professor Fernando Sanford, of Stanford University, describes a concentration cell in which the direction of deposition is the reverse of what would be expected if it were previously assumed that the solution tension of the metal is constant for both solvents. He offers an explanation connecting the phenomenon with the dielectric property of the solvent.

In the absence of quantitative data, the great difference known to exist between the solution tensions of a metal in different solvents would seem a sufficient explanation. It is true that in the Nernst theory of the concentration cell prior to 1894 it was supposed that the solution tension of a metal was a con-

stant property of the metal at a given temperature; but the supposition was short lived, as it involved a difficulty exactly like the one in question, and led to measurements of solution tension in water and in alcohol,¹ so that apparently a difficulty has been raised which does not exist.

It may well be, as Professor Sanford suggests, that there is a relation between solution tension and the inductivity of the solvent, just as there must be a relation between inductivity and dissociating power, since the forces between charged bodies vary inversely as was remarked by J. J. Thomson and by Nernst. The same consideration would indicate a relation between the effective solution pressure of a metal and inductivity, since there could hardly be a more typical condenser than the Helmholtz "double layer." Certainly the quantitative investigation of the matter is greatly to be desired.

An assumption of constancy of solution tension of a metal in contact with varying concentrations of its ions in the *same solvent* is not warranted; although the results of computations using the equation for electromotive force,

$$\pi = \frac{RT}{nF} \left(\ln \frac{P_1 \cdot p_2}{P_2 \cdot p_1} \right),$$

in which the solution tensions, P_1 and P_2 , are assumed to cancel, and the ionic concentrations, $m_1 \alpha_1$ and $m_2 \alpha_2$, are substituted for the osmotic pressures, p_1 and p_2 , would indicate that the simplified equation is at least approximately true.

On *a priori* grounds, the assumption is contradicted by the probability that the maintenance of ionization is largely due to an association of the charged particle with molecules of the already associated solvent, as well as that large inductivity and association certainly accompany each other, even if no simple relationship exists. So that it seems reasonable to expect, as he points out, that the inductivity of a solvent would change with changing concentration of ionic solute. But the change is in

¹ H. C. Jones, *Zeitschr. f. physik. Chem.*, 14, 346 (1894).

the opposite direction to that supposed in Professor Sanford's explanation, because increase in ion content must increase the inductivity of the solution, as will appear from the following consideration:

As ions pass from the metal into the solution, the changing composition of the mixture is accompanied by an increase in its density. The density, d , of a solution of any given concentration is related to its index of refraction of light, n , approximately as shown by the equation, $(n-1)/d=R$, the specific refractive power, a constant. A more concentrated solution, *i. e.*, a different proportion of the same components, which has a greater value for d , will also have a greater value for n , since the values of these physical properties depend additively upon the values of the same properties of the components. It would not be proper to substitute for n in the above expression the square root of the dielectric constant, as the electromagnetic theory might suggest, because the latter relationship is not capable of experimental test under the conditions for which the former is found to hold. But while the exact form of the function may be unknown, there can be no doubt that when refractive index increases as in the above case, the inductivity must increase also.

Applying this to a concentration cell, on the dilute side the inductivity of the solution is increasing, and this increment in the inductivity favors the further solution of the metal, but the osmotic pressure of the metallic ions is also increasing, and this increment opposes the further solution of the metal. Solution pressure, the predominating force on the dilute side, is aided by inductivity, and these together constitute a growing force—opposed, however, by a more rapidly growing force, osmotic pressure. In the more concentrated solution around the other electrode, we have an initially greater inductivity which is decreasing as metal ions are discharged and deposited, and this decrease of inductivity favors the deposition (or opposes the solution) of the metal; but the osmotic pressure of the metallic ions is decreasing also, and this decrease opposes the deposition. On this side, solution pressure

is aided by a relatively large but decreasing inductivity, and combined they constitute a diminishing force which is initially weaker than the opposing osmotic pressure, but stronger than the corresponding solution pressure of the other electrode. All of the pressure differences in the cell owe their existence to the difference in concentrations of the solutions, and all reach equilibrium when the concentrations become equal.

In formulating the total combined effects on both sides of the cell the inductivity effect is either added to the solution pressure or subtracted from the osmotic pressure of the cations in solution. We are not so much concerned here with the value of the ratio we call inductivity or its nature, as with its effect, which is a pressure. Let us call this the modulus i , then the familiar equation becomes

$$\pi = \pi_1 - \pi_2 = \frac{RT}{nF} \left(\ln \frac{P(1+i_1)}{p_1} \right) - \frac{RT}{nF} \left(\ln \frac{P(1+i_2)}{p_2} \right) = \frac{RT}{nF} \left(\ln \frac{p_2(1+i_1)}{p_1(1+i_2)} \right).$$

In this we have assumed, after all, that fundamentally the solution pressure is constant, but that there is a difference in *effective* solution pressure due to difference in inductivity. This seems reasonable where we are dealing with the same solvent as in a simple concentration cell: here the differences in inductivity are probably small. Would this equation suffice for different solvents in which i_1 and i_2 are unrelated, or must we still keep P_1 and P_2 distinct and find some further cause for a difference in solution tension of the same metal?

A series of inductivity measurements for varying concentrations of, say zinc sulphate, in water, with measurements of electromotive force of elements composed of zinc in the same concentrations of the salt, might lead to a clearer knowledge of the magnitude of solution tension, and might even throw some light on the as yet unknown forces whose resultant we call dissociating power.

In conclusion, allow me to say that this is not written in a spirit of controversy, but in order to place a little of our existing knowl-

edge at what may be a new angle to some one else who may thereby perceive a generalization or means to discover one. This, which seems to be the purpose of discussion, will be served as well even though I may have fallen into errors far more grievous than the apparent one that has occasioned this communication.

HORATIO HUGHES

THE TRUE SOIL SOLUTION

JUST recently, Dr. C. B. Lipman has published¹ a preliminary paper describing a "new method of extracting the soil solution," by subjecting the soil to a maximum direct pressure of 53,000 pounds to the square inch. This preliminary article describes briefly the apparatus used in obtaining this enormous pressure and claims for this new method the "obtaining of the soil solution *as it exists*"² in relatively thin films around the soil particles. The procedure is rapid, clean and of high efficiency. With further improvements in apparatus which we now are planning, the method should supplant all other methods known to-day, including even the Morgan procedure." The fault found with the Morgan method is that it is "laborious and slow, and introduces the factor of oil which complicates and renders it extremely time-consuming and untidy."

Let us look at the important points Dr. Lipman claims for his direct-pressure method.

It allows of the direct determination of the concentration of the soil solution, and of the amounts of each of the solutes contained therein.

The physical chemist is familiar with the fact that pressure is a considerable factor in influencing solubilities and it does not seem logical that a method employing such enormous pressures could obtain the soil solution "as it exists" in the soil without upsetting the whole physico-chemical equilibrium of the real soil solution; its specific gravity, viscosity, surface tension, osmotic pressure, spe-

cific conductivity and its chemical composition would all suffer more or less of a change which would combine to render the solution worthless to the plant physiologist or to the plant physiological pathologist from a scientific point of view. The reason that the soil solutions obtained by other methods are at fault is largely because the water added in extracting the soil changes the solubilities of certain of the ingredients. The van Suchtelen-Itano paraffin-oil displacement-pressure method described by Morgan³ was worked out carefully with just the opposite idea in mind, *i. e.*, to subject the soil to *as little pressure as possible* so as to preserve intact the physico-chemical equilibrium of the solution obtained. To this end the most inert oil was carefully selected as the displacement medium and pressures not exceeding 500 pounds per square inch were employed for forcing the oil into the soil. The preliminary tests⁴ of the paraffin-oil displacement-pressure method, run by van Suchtelen and Itano before extensive work was done by these investigators and by Morgan, show that the inactive paraffin oil when brought into intimate contact with the soil solution did not change the electrical conductivity, chemical composition nor surface tension. The solution is literally pushed out of the soil by the inert oil, only sufficient pressure being used to force the viscous oil into the soil.

The oil-pressure method is somewhat time-consuming, laborious and untidy, but common workmen after being carefully instructed can do this work under the supervision of the trained scientists; again, not one but a battery of as many cylinders as desired can be used to obtain sufficient quantities of solution in a minimum time. However if Dr. Lipman's above contentions did hold true in every respect the end in view, *i. e.*, the obtaining of a solution representing *most nearly in all re-*

¹ Lipman, C. B., "A New Method of Extracting the Soil Solution," Univ. of Calif. Publ. in Agr. Sciences, Vol. 3, No. 7, pp. 131-134, March 15, 1918.

² Italics ours.

³ Morgan, J. F., "The Soil Solution Obtained by the Oil Pressure Method," *Soil Science*, Vol. II., No. 6, 1917, pp. 531-545, Pl. 1.

⁴ Report of the Bacteriologist, 26th Annual Report of the Michigan State Board of Agriculture, pp. 152-153.